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MESSAGE(S): Jeff — My apologies for the delay in getting this to you. Included are responses to issues 1+3; 6 and citations to follow later today. I'm leaving for 2 weeks Ex. 6 - Personal Privacy Sunday so if you have any questions, please call today. Thanks, Gordon

If you do not receive all pages, please call back as soon as possible.

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FS-6200-28(4/88)

Review of 'white papers' on Oregon Forest Practice Rules as they pertain to Oregon's Coastal Salmon Restoration Initiative[1]

Background: This review was requested by the National Marine Fisheries Service to provide information for their evaluation of the Oregon Forest Practice Rules as a component of the Coastal Salmon Restoration Initiative. Information reviewed was provided by NMFS and included the NMFS white paper ('Appendix 1') identifying six issue areas, and the white papers prepared by the Oregon Department of Forestry in response to these issues. This reviewer was specifically asked to review issues 1 (landslides and debris flows), 3 (hydrologic changes), and 6 (sediment and other risks from roads). Other information provided by NMFS included the Oregon Dept. of Forestry Forest Practice Administration Rules (Chapter 629), dated January 1997 and review questions for each of the 3 issues addressed.

General comments: Before addressing the specific questions requested, some general comments on the limitations of this type of review and review request seem in order. In reading both the NMFS white paper and the ODF response, I was struck by the vague and abstract nature of the debate being joined on both sides. Both sets of papers reflect a rather selective reading of the relevant scientific literature, and seem more intent on substantiating positions already taken than on a balanced weighing of scientific evidence.

It is difficult for a reviewer to evaluate this kind of data-free debate without introducing yet another set of biases. I have tried to be as objective as possible in my comments. However, a more useful way to conduct future reviews, where there are significant differences of opinion on science policy questions, would be to convene an independent panel of experts and let them conduct the review process *in toto*, soliciting the comments of others as necessary. This is the kind of review process used by NSF and other science-based agencies to evaluate proposals and programs, and I see no reason why it shouldn't be used for policy questions as well. This would eliminate the multi-layered reviews of reviews of reviews that the current process provides.

Rather than respond on a point-by-point basis to the many issues discussed in the white papers, I have tried to distill the arguments down to the key areas of agreement and disagreement and respond to these.

Specific comments:

Issue 1. Risks posed by landslides and debris flows

The NMFS white paper outlines a number of issues focused on the potential detrimental effects of landslides to channels and aquatic habitat. Key issues include:
1) ODF rules do not explicitly restrict harvest activities on unstable slopes;
2) clearcutting alone, particularly in unstable headwalls, can dramatically increase the landslide rate;

[1]Prepared by Gordon E. Grant, Research Hydrologist, PNW Research Station, 3200 Jefferson Way, Corvallis, OR, 97330

3) no procedure for determining the delivery of landslide and debris flows-generated sediment to fish-bearing streams is utilized under the ODF rules;
4) uncertainty exists as to how well geo-technical specialists can identify high-risk sites;
5) sites with moderate slide potential, which are unprotected under ODF rules, can become unstable following cutting and deserve protection.

Implicit in the NMFS discussion is the assumption that landslides and debris flows are, by and large, detrimental to fish and fish habitat, and active measures to reduce land-use accelerated landsliding are required to protect fisheries values. This is a common, but not universally shared view. In sediment-limited streams, such as the Oregon Coast Range, landslides may be the primary mechanism by which key structural elements forming channel habitat, i.e., wood and sediment, enter the channel system (Reeves et al, 199). On the other hand, the importance of landslides and debris flows as sources of sediment and wood may be exaggerated in the modern, managed landscape, where streamside logging and reduction in the size of wood entering channels limits opportunity for wood and sediment recruitment under less catastrophic conditions.

The ODF paper takes exception with these conclusions, and takes the position that:
1) In-unit failures are not a significant source of landslides, since loss of root strength does not significantly affect slope stability; this view is supported by high failure rates in headwall leave areas, where trees are left to provide slope stability;
2) Over the long-term (multiple rotations), the in-unit failure rate is approaches the 'natural' (i.e., unharvested) rate;
3) Roads are acknowledged as a major factor contributing to accelerated landsliding and debris flows in logged areas in the past, but newer road construction practices, and new knowledge and techniques for identifying high risk sites are adequate to reduce risk of road-initiated failures without additional rules;
4) Landslides can have both positive and negative effects; wildfire may have intiated periods of landsliding in the past but since it is now suppressed, some increased sliding due to harvesting is consistent with the 'natural' disturbance regime.

My response:

1) There have been *many* studies over the past 25 years, conducted by academic, state, federal, and industry scientists, that have documented an increased risk of landsliding from clearcuts alone. Most of these studies have employed aerial photographic techniques with some ground-truthing. While detection of slides under closed canopies is an issue where only air photos are employed, enough studies have incorporated field checking to insure a reasonably high level of detection and representation of slide rates under all landscape conditions (i.e., Swanson and Dyrness, 1975; Swanson and others 1977).

In general, the increased rate of sliding extends from one to two decades following harvest; the exact timing of this 'window of vulnerability' is consistent with the model of gradually reduced root strength as roots decay. Typically, fine roots decay within the first 2-3 years with larger roots decaying over the next 5-15 years, with roots in wetter sites (i.e., Oregon Coast) decaying before drier sites (i.e., Idaho Batholith). Typically, landsliding from clearcuts or plantations < 20 years old increases 2-4 times the forested rate. Recent studies following the February 1996 flood are documenting that sliding rates from >20 year-old plantations in the western Cascades are returning to background (forest) rates. This evidence, too, is consistent, with a recovering root strength model as a primary mechanism.

The discussion about root strength seems beside the point. There is quite a bit of evidence from both field and laboratory studies that roots *do* impart additional cohesion to soils. Even without penetrating the failure surface, roots provide lateral strength to soils, thereby increasing slope stability. Field studies following slides typically (but not always) show broken roots, clear evidence that root strength increased cohesion. But even if root strength is not a factor in all slides, the unequivocal empirical evidence from slide inventories that slide rates are increased in clearcuts means that the issue of cutting on potentially unstable slopes needs to be addressed. Even the studies cited in the ODF paper report that approximately one-third of landslides monitored since 1988 occurred in units.

High rates of sliding in the few studies that have examined headwall leave areas are difficult to interpret. Headwall leave areas in coastal Oregon were only instituted on sites that were interpreted as having a high potential to slide and have adverse consequences to streams. Consequently they do not represent an unbiased sample of headwall areas, but are likely to have increased slide potential relative to surrounding areas. A better study design for interpreting headwall (or other landscape factors) contribution to slope stability would be to stratify the landscape by intrinsic slope stability (i.e., rock type, slope steepness, soil depth) and then examine sliding rates by area in forest, clearcut, and road. On balance, however, headwall leave areas have not been shown to significantly increase slope stability. In general there has been very little study of how varying the pattern or level of green trees retained on a slope following harvest affects slope stability.

2) There have been virtually no studies to date showing that the long-term sliding rates from clearcuts approaches that from unharvested areas. This is a difficult proposition to support (or refute), since we don't have slide inventories conducted over multiple rotations. In general, slide frequencies are strongly affected by the timing and intensity of large storms that may occur only once every 30 - 50 years; disentangling the effects of storm timing relative to harvest is problematic. Current post-mortems on the February 1996 promise to shed some light on this issue, but the jury is still out.

3) There is common agreement that roads are a major factor contributing to increased slides from harvested areas. Factors include road design and construction techniques that result in destabilized slopes, road drainage problems that increase water delivered to slide-prone areas, and road/stream network interactions at stream crossings (i.e., plugged culverts) that initiate slides and debris flows.

Our current ability to identify high risk sites has not received adequate testing and validation, however. The ODF paper offers no evidence that their criteria for identifying unstable sites, which are quite broad and include field evidence for both shallow and deep-seated mass movements, are adequate in the face of slide-triggering storms. Slope stability mapping is not currently required on state and private land, and there are no retrospective or prospective studies (to my knowledge) that have substantiated the premise that geotechnical specialists can identify high hazard sites. Existing state-of-the-art landslide prediction models (i.e., Montgomery and Dietrich) are severely limited by the resolution of digital terrain data and lack of information on soil depth and properties. More intensive. site-specific models (i.e., LISA) are too data intensive to be widely applied.

Current flood studies by both ODF and PNW are documenting the full range of these interactions and hold the promise of improved practices in the future. While these studies suggest that slide rates from roads during the most recent storms were less than in previous large floods, it is not clear to what extent new road construction techniques or road restoration practices are contributing to this reduced frequency. Until the past year, there have not been a enough of storms of sufficient magnitude to test new practices, so evidence to support the effectiveness of these measures awaits the conclusion of this work. Another key point is that even though road construction practices may have changed for the better over the past 20 - 30 years, a legacy of old roads are still present on the landscape, contributing to slope stability problems. Assessment of the risk of future landsliding requires identifying where and in what condition these roads are.

4) The ODF paper makes an important point, neglected in the NMFS paper, that landslide rates need to be considered in the context of the natural disturbance regime. See discussion above under the NMFS paper summary. There have been very few studies that have attempted to contrast the pre-management sliding rate (including wildfire) with the post-management rate (including wildfire suppression and logging). The issue is not just the rate itself; but also the spatial pattern -- are the same parts of watersheds being influenced by sliding and debris flows? Research on these issues is still quite rudimentary, although current modeling efforts (i.e., Benda, CLAMS) promise new insights in the next few years.

Neither the NMFS nor the ODF paper get at the heart of the problem -- that landslides and debris flows are a natural and inevitable consequence of steep mountain terrain and high rainfall intensities, with or without timber harvest, but that the frequency, spatial pattern, and possibly behavior of these processes may be influenced by human activities in such a way as to pose risks to salmonids and other aquatic organisms. There is good evidence that cutting and road construction increase slide frequencies, but we have much less understanding of how the pattern or behavior may have changed. We are inherently limited in our ability to precisely predict where landslides and debris flows are likely to occur, and how far they are likely to go -- even our most sophisticated models can only provide rough estimates of probabilities. Finally, while there is evidence that landslides and debris flows provide both positive and negative benefits to fish (who have presumably adaptively evolved to river systems that experience episodic disturbances), we are unable to state with confidence what the consequences of a particular frequency or pattern of disturbances are likely to be.

Issue 3: Potential hydrologic change from land use activities.

The issue of potential hydrologic change caused by forest harvest activities has been intensely debated for many decades without resolution. As pointed out by both the ODF and NMFS papers, the many studies conducted show a wide range of results, with increases, decreases, and no change to both peak and low flows reported. The factors that determine how a given landscape will respond to a given treatment are not well understood, and we are not able to predict with confidence either the direction or the magnitude of the resulting change. This is not entirely surprising, given the variety of potential hydrologic mechanisms linking harvest activities to streamflow (i.e., ODF, pg. 2). Recent floods in the PNW have intensified this debate, in part because increased public and media attention have coincided with the publication of new studies that suggest that timber harvest may result in more persistent and pervasive hydrologic changes to peak flows than previously thought.

The NMFS paper raises a number of potential hydrologic changes due to timber harvest but does not attempt to constrain either their geographic scope, magnitude, duration, or consequence(s) for fisheries. Most of the hydrologic effects described come from research conducted in small watersheds; it is not at all clear to what extent these results apply to larger basins. NMFS makes no mention of recent work (i.e., Jones and Grant, 1996; Wemple and others, 1996) that suggests that hydrologic changes due to harvest might apply in larger basins.

The ODF position appears to rest on the following points:

1) Studies showing hydrologic changes due to logging are inconclusive or show no effect; if present, such changes only affect small storms that don't affect channel morphology and fish habitat, and in any case are likely to be less than hydrologic changes following natural disturbances, such as wildfire;
2) Past studies do not take into account changing practices, and current Best Management Practices (BMPs), particularly for roads, are sufficient to address any potential hydrologic consequences;
3) Other land use practices, such as urbanization and water withdrawal, result in much more significant hydrologic change; hence the focus on forest activities is not warranted.

My views on these three points:

1) Studies linking hydrologic changes to logging *are* inconclusive, in the sense that a wide range responses are reported; these responses vary depending on whether one is looking at peak flows, low flows, total water yields, or other measures of hydrologic change, and also vary depending on geography, type and intensity of treatment, etc. Effects of timber harvest activities on streamflow are clearly present in small watersheds, however, and may be present in much larger basins. In a recent study that represents the longest and most comprehensive retrospective

analysis to date of streamflow records in small (<1 km^2) experimental and larger (60 - 600 km^2) basins (but was not cited by either the NMFS or ODF papers), Jones and Grant (1996) reported peak flow increases of 30-50% in both sizes of watersheds in response to 100% clearcutting or 25% cutting with roads. Consistent with other studies, the increases are greatest for small to moderate-sized storms (< 1 year return periods) but appear to be true for larger events as well. It is difficult to say anything about the effects of management on the largest floods, such as occurred in 1964 and 1996, because of the limited number of events during the period of record. Current hydrologic thinking is that the effects of management are likely to be diminished or absent in the case of the largest events, particularly for large basins. This has never been demonstrated conclusively, however, and in fact we observed 20 - 40% higher streamflows from 20 - 30 year old plantations in the H.J. Andrews Forest during the February 1996 storm (Dyrness, in review). Jones and Grant (1996) along with Wemple and others (1996) postulated that the road networks may be playing a particularly important role in incresing the hydraulic routing efficiency of logged basins, but this has not been proven.

While the jury is still out in terms of the effects of harvest on the largest storms, there is common agreement that, in mountain watersheds at least, the infrequent storms (> 10 year return period) have the greatest impact on channel morphology, in terms of moving most of the bedload and restructuring the channel bed. Flows of a lesser magnitude, however, can still play an important role in terms of transporting sediment (Grant and Wolff, 1991), wood (Lienkaemper and Swanson,), channel changes (Nakamura and Swanson), and scour and fill cycles influencing the survival of salmonid embryos (Montgomery and others, 1996). While potential harvest impacts resulting in peak flow changes that alter fish and fish habitat have never been shown directly, they can also not be dismissed out of hand.

Peak flow changes associated with wildfire have never been demonstrated in western Oregon. Most examples of increased flows following fires come from chapparal or drier sites (i.e., Idaho Batholith), where intense fires can produce hydrophobic (water repellent) soils that cause very high overland flow rates. To my knowledge, no examples of hydrophobic soils following fire have been reported from western Oregon/Coast Range fires. Some increase in fall flows due to reduced evapotranspiration would be expected following fire, however.

Both the NMFS and ODF papers seem to focus more on the peak flow than the low flow aspects of flow modification. From a fisheries standpoint, however, low flow changes may be as critical, if not more so, since they occur during high stress periods (late summer) and are directly correlated with stream temperatures. The most detailed word to date on this question by Hicks and others (1991) is not cited by either document. They found that low flows typically increase by as much as 150% for a period of 5-7 years following harvest, but then decrease below the pre-harvest levels for a longer period (10-20 years). One possible explanation is the regrowth of riparian, phreatophytic vegetation (i.e., *Alnus* sp.). It is not clear how pervasive or persistent this effect is, but it probably deserves some attention.

2) The ODF paper takes the position that BMPs applied to roads *primarily to reduce sediment*

effects [emphasis mine], are also adequate to reduce any potential hydrologic changes associated with roads. The argument seems to be that road practices that keep sediment from moving into streams also function to reduce streamflows. This proposition is entirely untested, does not address the hydrologic effects of the road itself, and seems problematic on its face. BMPs designed to reduce sediment impacts, i.e., reduce landslides, gullies, etc. do not alter either the road surface or cutbank subsurface interception, where road-related overland flow can be generated (Megahan, 1972; Wemple and others, 1996). In fact, most BMPs are designed to deliver road-generated or intercepted water to a stream, rather than return it to the hillslope, where it might cause gullying or slope instability. This has the effect of increasing the runoff rate. To my knowledge, there has been no direct monitoring of road-related hydrologic effects, including the most recent ODF study on the February floods.

Recent, though unpublished work, by Wemple (Ph.D. dissertation, Dept. Of Forest Science, OSU) is demonstrating that there is considerable variability in how roads respond during stormflow. Factors contributing to this variation include the road's position relative to 'native' streamflow generation mechanisms, and the efficiency of the ditch and gully system in delivering excess water to streams.

3). The ODF point that other land use practices, including urbanization and water withdrawal, have more pronounced and immediate consequences for streamflow than timber harvest is generally true. The magnitude of peak flow increases observed in large basins due to harvest activities alone appears to be less than the year-to-year or even storm-to-storm variability (Jones and Grant, 1996). Streams in urbanizing areas show much more pronounced streamflow responses to increases in impervious area (Booth, 1990). The effects of reservoir regulation are typically even more extreme (Grant, 1997). But all of this seems to miss the point -- that there are real and demonstrable changes in hydrology due to harvest activities alone. The key question again is the significance of these changes for aquatic organisms. Neither the NMFS nor the ODF paper really address this point.